

THE UNIVERSITY OF WISCONSIN  
MADISON 6

DEPARTMENT OF MEDICAL GENETICS  
SCHOOL OF MEDICINE

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GENETICS BUILDING

DEPARTMENT OF GENETICS  
COLLEGE OF AGRICULTURE

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Dear Gus:

To yours of June 23. (1958)

Sorry to take so long in replying, but I've been rather tied up, especially in last minute arrangements for the following. Barring some unlikely mishap, we are planning to move to the Stanford Medical School to organize a Department of Genetics, along somewhat similar lines to the set up here. Some of the strong research people we would be closely associated with would be Kornberg (DNA Biochemistry); Henry Kaplan ("radiology"); Sid Raffel (immunology); C. B. Van Niel (microbiology); Cliff Grobstein (embryology)... Palo Alto, the town, is about 20 miles south of San Francisco and has the reputation of being an ideal place to live and work. It is also a reasonable distance from Berkely, across the bay from San Francisco.

This is a new development; since our stay in Australia - then, as you know, we were in negotiations with Berkeley, but this is a far more congenial situation and we are especially looking forward to close dealings with Kernbergs's group, who are forming the Biochemistry Department, moving from Washington University, St. Louis. One of the people in this, by the way, is Mel Coln (that "cove") who shares your interest in antibody formation by single cells and whom I hope you get to know and like.

I am sure you are going to enjoy the setting there very much when you come for your fellowship. I'm looking forward to discussing this with Sir Mac, O.B.E. (Believe it or not we had heard of the award over here.

We will probably set up at Stanford next July after a 3-months interim with Cavalli in Italy. (April-June).

Have you seen the editorial in Lancet? (June 21, 1958, p. 1321-2). They were kind enough to send me a tear sheet.

Gus, I would like to see your other mss. very much, but just now's impossible. (see enclosed itinerary!) Send what you have this fall.

I am disappointed in your rejection of statistical analysis because the "data are too thin" - that's just when you need it most, to bolster your intuition with some hard numbers! The problem is not so very difficult. I don't thoroughly agree with you about how far to go in pushing the cell experiments, but I agree that for now it would be better to stress methodological improvements rather than just the testing of more cells.

Did you ever locate the Moeschlin reference?

As ever,  
